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Interactive
Comment

Interactive comment on "3-D thermo-mechanical laboratory modelling of plate-tectonics" *by* D. Boutelier and O. Oncken

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This manuscript presents and discusses a sophisticated new modelling apparatus for three-dimensional thermo-mechanical laboratory modelling of plate-scale geodynamic processes such as subduction. The authors present three quasi-two-dimensional experiments of subduction to illustrate how the experimental technique works, and what sort of quantitative data can be extracted from the experiments (most notably stresses in the overriding plate and along the subduction interface) . The experiments involve a subducting plate, overriding plate and underlying "asthenosphere", and convergence is driven by a velocity boundary condition at the back of the subducting plate (piston). The external control on the convergence velocity is required to properly scale for thermal diffusivity. As such, the modelling technique does not allow for fully dynamic experiments to be conducted, which could be considered as a disadvantage. Nevertheless, the models do have the added advantage of incorporating temperature effects, which most fully dynamic laboratory models of subduction do not incorporate.

This paper will be of interest to geologists and geophysicists working on subduction zones, in particular geodynamic modellers. I have a few comments (see below) that I think require some attention. My most significant comments regard the usage of water as an analogue for the sub-lithospheric mantle (point 2), and the reported 2D set-up and potential boundary effects in the experiments (point 4). In any case, I think that the authors have developed a very sophisticated modelling apparatus and I look forward to their future 3D modelling work on subduction zone processes.

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COMMENTS

1. In the introduction the authors could cite some earlier work of thermomechanical laboratory modelling of subduction, e.g. Jacoby [Tectonophysics, 1976] and Kincaid and Olson [JGR, 1987].

2. Page 108, lines 5-11: "Since the viscosity of the asthenosphere is several orders of magnitude lower than the effective viscosity of the lithosphere (Mitrovica and Forte, 2004; James et al., 2009), it can only exert a small shear traction on the base of the lithosphere (Bokelmann and Silver, 2002; Bird et al., 2008), which can be neglected if we focus our interest on the solid-mechanics interaction of the plates in the subduction zone. Consequently the asthenosphere can be modeled with a low-viscosity fluid (water) whose unique role is to provide hydrostatic equilibrium below the lithosphere.". The authors discuss the viscosity ratio between the asthenosphere and the lithosphere, but it is not entirely clear to me if they refer to the low-velocity zone from ∼100-250 km depth or to the entire sub-lithospheric upper mantle. The model depth in their experiments (25 cm) actually scales to 875 km depth in nature (with 1 cm representing 35 km), which includes the entire upper mantle and the uppermost ∼200 km of the lower

mantle. Apart from the point mentioned above, I think this part requires a little more discussion. It might be that the sub-lithospheric upper mantle exerts negligible shear traction at the base of the plates, but there is also the component of shear traction exerted along the top surface and bottom surface of the subducted slab. And another important component is the normal traction exerted by the ambient mantle on the slab as it migrates in a direction perpendicular to its surface (e.g. during slab rollback or slab roll-forward or during slab steepening). These normal and shear tractions on the slab are most likely very significant in nature, but in the models presented here using water they are negligible because of the very large viscosity ratio used. I would estimate the viscosity ratio in the experiments presented by the authors to be around 10e6 - 10e7, with an effective viscosity of the subducting plate of 10e3 - 10e4 Pa s and that of water of 10e-3 Pa s. Some recent works argue that the subducted slab/upper mantle effective viscosity ratio is much lower than that, of the order 100-1000 [e.g. Moresi and Gurnis, EPSL 1996; King et al., PEPI 2001; Capitanio et al., EPSL 2007; Wu et al., EPSL 2008; Schellart, G-cubed 2008; Funiciello et al., EPSL 2008; Loiselet et al., Geology 2009; Stegman et al., Tectonophysics 2010]. In some of my own recent works [Schellart, EPSL 2009, EPSL 2010, JGR 2010] I have estimated the energy dissipation in the subducting slab during progressive subduction of a high-viscosity plate into a low-viscosity upper mantle reservoir (with slab/upper mantle viscosity ratios of 66-1375). These works indicated that, during the initial sinking stage before the slab tip arrives at the 670 discontinuity, up to 6-22% of the potential energy of the slab is used to bend the slab at the subduction zone hinge, and that not more than 1% is used to curve the slab in plan-view. The experiments thus indicate that some 7-23% of the potential energy of the sinking slab is used to deform the slab, while the remainder (∼77-93%) is used to drive flow in the ambient mantle. Although it should be added that these models do not include an overriding plate, it is expected that energy dissipation in the overriding plate would amount to only a few %, suggesting that, in nature, some 74-90% of the potential energy of the sinking slab is used to drive mantle flow. This is in stark contrast to what would be expected from the experiments presented by the authors, where virtually all

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the energy is used to deform the plates. The authors should discuss how this affects their model results, e.g. in case a more realistic (higher) sub-lithospheric upper mantle viscosity would be used and a large percentage (∼74-90%) of the potential energy of the slab would be dissipated in the sub-lithospheric mantle in their models, would they still expect a similar style of subduction, similar stresses in the overriding plate, similar cylindrical geometry of the slab, etc. For example, I would expect that the slab would be deformed in a non-cylindrical fashion for higher ambient mantle viscosities.

3. Page 111, lines 15-17: I'm assuming these are in weight %?

4. Page 115, lines 4-5: "The experimental tank is sufficiently larger than the model plates (5 cm on each side) that the sides can be considered to be free.". And Page 117, lines 17-18: "The experiments can thus be considered two-dimensional despite the relatively large model width.". And also page 123, lines 4-6: "The presented experiments are large but two-dimensional models since we did not implement any lateral (i.e. along-strike) variations of the model structure, mechanical properties or any other initial or boundary conditions.". Regarding the statement that these models are two-dimensional: In a strict sense, the models are not two-dimensional, because that would require them to be infinite in trench-parallel extent. The fact that the model plates have lateral edges makes the model three-dimensional. Even though the slab retains an approximately cylindrical shape during subduction, return flow of the water around the lateral slab edges is expected (through the 5 cm gaps), so flow in the experiments is also not 2D. I think the authors should call these models quasi-two-dimensional, but not two-dimensional. Then, regarding the statement that the sides can be considered free: This is mostly the case because a very low viscosity fluid is used as the sub-lithospheric mantle (water), which is probably 6-7 orders of magnitude less viscous than the effective viscosity of the plates, and therefore energy dissipation in the water is negligible throughout the experiments and virtually all energy is dissipated in the plates. However, in case a fluid would be used with a higher (more realistic) viscosity (as the authors suggest to do in future work) (for example 10e4 times higher to get a

slab/upper mantle viscosity ratio of 100-1000), then the sides cannot be considered free. The fluid would then experience considerable drag along the lateral boundaries of the box, and the lateral slab edges would thus feel, and be influenced by, these lateral boundaries. In low-Reynolds number flows («1), the sinking velocity of an object in a fluid container is already reduced by up to 2% (compared to an infinite volume reservoir) in case the tank diameter is 100 times that of the object diameter [Tritton, 1977]. In the models presented by the authors, the box width is only 1.25 times that of the slab. Although it would be highly impractical to do subduction experiments in a tank that is 100 times the width of the slab (so 100 x 40 cm in this case), it is also not required because of the other important length scale involved in the subduction process, namely the box depth. In any case, because the toroidal mantle return flow patterns during lateral slab migration approximately scale with the half width of the slab [Schellart et al., Science, 2010], a good guideline to minimise influence of the lateral boundaries of the box would be to have a box width that is at least twice the slab width. I suggest that the authors provide some discussion on this.

5. What is the typical Reynolds number in the experiments? It seems to me that it is rather high (Re > 1). With a density of 1000 kg/m3, a typical velocity of ∼0.25 mm/s, a length scale of 0.02 m (thickness of plate) or 0.40 m (width of plate) and a viscosity of water of 0.001 Pa s, I get Re = 5 (with 2 cm) and Re = 100 (with 40 cm). Thus, inertial effects play a significant role in the water reservoir, and there might be unsteady (turbulent) flow in the water, for example near the lateral slab edges. Considering that the total amount of energy that is dissipated in the water is most likely very low and negligible compared to the energy dissipation in the plates, and considering that attention here is focussed on deformation of the plates (and not flow patterns in the mantle), such unsteady flow might not be all that relevant here. I would like to stress, though, that for future work the authors should keep in mind that for proper dynamical scaling of the models and for properly modelling the flow patterns in the mantle, Re « 1.

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6. Page 118, line 1: What is the accuracy in density for the lithosphere and the asthenosphere? It would be good to provide some error margins here.

7. Page 118, line 2: "the plate boundary is lubricated (tau = 0).". Lubricated with what? Please expand. Does tau = 0 mean shear stress is 0 Pa? Please explain. It would also be useful to provide information on the lubricant here (e.g. linear-viscous with viscosity of .... Pa s) and the estimated thickness of the lubricating layer ($\sim$1 mm / less / more / ....).

8. Page 118, line 22, equation: I think "cos(alpha)" should be "sin(alpha)", because with alpha = 0 then Fph = 0 and with alpha = 90 degrees then Fp = Fph.

9. Page 125, lines 9-10: "However the density should not be too high or trench-perpendicular tension would be produced.". This reads a bit as if trench-perpendicular (deviatoric) tension is an unwanted effect because it is unrealistic, which it is not, otherwise there would be no backarc basins opening up. Please rephrase.

10. There are several errors in Table 1 on page 134: *For the Elastic shear modulus, the values for the model (10e10 Pa) and nature (10e3 Pa) do not give the scaling factor (8.79 x 10e-8). *For the asthenosphere density the scaling factor should be 0.308 (and not 3.25). *For time the values for model (3.15 x 10e13) and nature (9.2 x 10e1) do not make any sense. Are they the wrong way around?

11. In Figures 7, 9, 11 and 12, please add experimental times to each of the individual images.

SPELLING / GRAMMAR

Page 106, lines 4-5: "......... made of temperature-sensitive elasto-plastic with softening analogue materials ....". Do you mean "............ made of temperature-sensitive elasto-plastic analogue materials with strain softening ...."?

Page 106, line 22: "phenomena".

Page 118, line 5: "homogeneous".

Page 119, line 1: "than", not "that".

Page 119, line 1: "asthenosphere".

Page 119, line 15: "slab".

Page 121, line 28: "dies". Maybe replace with "becomes inactive"? Sounds a bit less dramatic.

Page 123, line 17: "rely on".

Page 125, line 5: "nature" (no capital).

Page 126, lines 2-3: "fundamentally".

Page 126, line 26: "a" not "an".

Interactive comment on Solid Earth Discuss., 3, 105, 2011.

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